STANFORD UNIVERSITY
STANFORD, CALIFORNIA 94305

DEPARTMENT OF CHEMISTRY

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Dr. Robert Olby
Department of Philosophy
University of Leeds
LS2 9JT
England

Dear Dr. Olby:

I thank you for your letter and for the accompanying sections of your article.

I think that some revisions should be made in your writing. Near the bottom of page 17 you mention that Crick and I met at Easter time in 1952. In fact, we did not meet then, because I was prevented from attending the Royal Society Symposium on 1 May 1952 by my inability to get a passport issued to me by the State Department. Corey was there, and it is possible that Crick and Corey talked about the coiled-coil structures, and that Corey said that he was not working on the problem.

On page 17 you say that Crick told me that he was working on this problem and asked whether I was also, and that I replied that I was not.

What happened is that I went to England later on, in July or August, I believe (I do not have my diary at hand, but I could look up the exact date later this summer.). I remember what happened. Crick did not tell me that he was working on the problem. He simply said to me "Have you thought of the possibility that alpha helixes are coiled around one another?" I answered "Yes, I have." He did not say anything more, nor did I. I assumed that he had been working on this idea, as I had. The coiled-coil explanation of some of the features of the x-ray diagrams had occurred to me, and I had found that the idea was satisfactory except for one puzzling feature, which I continued to think about. When I had got this point clarified, shortly after my return to Pasadena that summer, Cory and I carried out some calculations to test the idea, and we were accordingly able to write our note to Nature and get it off on 14 October.

It is accordingly the three lines at the bottom of page 17 that seem to me to require revision. It was during the summer that Crick spoke with me. He did not say that he was working on the problem, although I surmised that he was. He did not ask whether I was working on the problem, but asked if I had thought of the idea of coiling the helixes around each other. I replied that I had thought of the idea. I was in fact working on it at the time, and had nearly completed my arguments at the time. I did not tell Crick what I was doing, because there was one aspect of the problem that still troubled me, and I was not sure then as to how it would be resolved. I assumed that Crick did not continue the discussion for essentially the same reasons; namely, that he had not completed his own analysis in a way that satisfied him and would permit him to publish it (by communicating it to me). I was not surprised by Crick's paper, when it appeared; but I was surprised, and irritated, that Nature should not have published the two papers together.

On page 18 you should, I think, refer to the paper as Pauling and Corey's, in line 7, rather than Pauling's; also, in line 10, and in the next paragraph.

Your additional note on the alpha helix seems to me to be correct. I had formulated the alpha helix and gamma helix structures in March of 1948. I had then put Dr. Branson at work on a more thorough analysis of the geometric problem. By 1950 I was pretty well convinced that other helical structures would turn out to be unstable. Corey and I decided to publish in JACS. As I recall, we were working on our PNAS papers when I saw the work of Bamford, Hanby and Happey.

Cordially,

Linus Pauling

LP:cb